ASTRONOMICAL BIBLIOGRAPHIES

PROF. HOLDEN, of the Observatory of Washington, U.S., lately read to the Washington Philosophical Society a paper on "Special Astronomical Bibliographies," in which a careful comparison of one section of the "Reference Catalogue of Scientific Papers," by Mr. E. B. Knobel, F.R.A.S. (lately mentioned in these columns), was made with a MSS. catalogue on the same subject (nebulæ and clusters) which Prof. Holden had compiled for his own use. Only one misprint was found (*op. cit.*, p. 377, for *Comptes Rendus*, vol. 28, p. 537, read p. 573) and only one omission of a highly important paper, the "Siderum Nebulosorum Observationes, Havniensis" of D'Arrest. The list of works given by Mr. Knobel might be still further extended, but in its present state it is very accurate and extremely useful.

A list of bibliographical works available to the astronomer for consultation is given, which we extract:—

1. WEIDLER, "Bibliographia Astronomica," &c., 1755, 8vo.
2. SCHEIBEL, "Astronomische Bibliographie," &c., 1789-98, 8vo.
3. LALANDE, "Bibliographie Astronomique," &c., 1803, 4to.
4. REUSS, "Repertorium Commentationum," &c., vol. v. 1804, 4to.
5. YOUNG, "Natural Philosophy," vol. ii. pp. 87-520, 2 vols., 1807, 4to.
6. SOHNKE, "Bibliotheca Mathematica," &c., 1854, 8vo.
7. [SCHUMACHER], "Catalogue des Livres Composant la Bibliothèque de Feu," Prof. Schumacher, Part 1, 1855, 8vo.
8. STRUVE, "Catalogus Librorum in Bibl. Spec. Pulkovensis," 1858, 8vo.
9. ROYAL SOCIETY, "Catalogue of Scientific Papers," 6 vols., 1867-72, 4to.
10. DARBOUX et HOUEL, *Bulletin des Sciences*, &c., 8vo. (*serial*).
11. POGGENDORF, "Biog.-Liter. Handwörterbuch," 2 vols., 1863, 8vo.
12. R. WOLF, *Sonnenflecken-Literatur*, *Astr. Mit.*, 8vo. (*serial*).
13. R. WOLF, "Handbuch d. Mathematik," &c., 2 vols., 1872, 8vo.
14. CARL, "Die Principien d. astr. Instrumentenkunde," 1863, 8vo., p. 161 [Literature of Micrometers].
15. BELGIUM ACADEMY OF SCIENCES, "Bibliographie Académique," 1875, 8vo.
16. ST. PETERSBURG ACADEMY OF SCIENCES, "Tableau Général," &c., Part 1, 1874, 8vo.
17. ENGELMANN, "Literatur d. astronomische Nachrichten," &c., Bessel's Abhandlungen, 1876, 3 vols., 4to.
18. KNOBEL, "Reference Catalogue of Astronomical Papers and Researches." *Mon. Not.* R.A.S. 1876, Nov.
19. HOLDEN, "Index Catalogue of Works on Nebulæ and Clusters of Stars, 1876," MS.

Almost any desired paper may be at once found by means of these works, and Nos. 3, 4, 8, 9, 11, 16, 17, and 18 are practically indispensable.

MENDELEEF'S RESEARCHES ON MARIOTTE'S LAW

IN compliance with your request I hasten to make known, through your esteemed journal, to the scientific public of England, the results of my researches on the Boyle-Mariotte Law. Special reasons which I have explained in the Russian *Journal of Artillery* (August, 1872), urged me to undertake new researches on this law. They are briefly as follows:—

1. It is impossible to admit in theory that under pressures infinitely great gases can be condensed into a volume infinitely small; or, in other words, that it is possible to introduce into a given volume an infinitely great mass of condensed gas. Although infinitely great pressures be practically unrealisable they are perfectly perceptible to the mind; as to an infinite condensation of matter it is quite inconceivable, otherwise we must admit the existence of an atomic substance without volume. The experiment of Cagniard-Latour developed and verified by MM. Wolf Drion, Andrews, and myself lead us to an inevitable conclusion, viz., that at a certain known temperature all gases and vapours can no longer be transformed under any pressure into liquid, but remain in the gaseous state, endowed with elasticity, but deprived of cohesion. At a lower temperature the gas may be transformed into liquid; but at a higher temperature it remains gas, whatever be the pressure. I gave to this temperature (*Lieb. Ann.*, t. 119, p. 11) in 1860 the name of "temperature of absolute ebullition;" in 1872 Dr. Andrews gave it the name of "critical point."

Imagine a mass of gas at a temperature higher than this, and suppose that this mass is subjected to pressures always increasing; if the Boyle-Mariotte Law is accurate the volume ought to diminish in inverse proportion to the pressure. If we represent this same mass of gas at a temperature a little less than that of absolute ebullition, the gas transformed into liquid will cease to be compressed as before, as the Boyle-Mariotte Law requires. Consequently, there is no doubt that we ought to come to the paradoxical conclusion:—A gas can be more compressed than a liquid or solid. As there is reason to believe that oxygen at the ordinary temperature is hotter than at its temperature of absolute ebullition, the Boyle-Mariotte Law being admitted, under a pressure of 2,000 atmospheres, we ought to find a specific weight of oxygen greater than that of sulphuric acid, and the pressure being 10,000 atmospheres, its density would reach that of mercury. But it is impossible to admit this, judging from what we know of the relation which subsists between the atomic weight of the elements and their density in the free state, as well as in their combinations with other elements. It is sufficient to indicate that a very great density is the peculiar property of combinations, the elements of which are endowed with a considerable atomic weight. Consequently, it cannot be admitted that elements having so small an atomic weight as oxygen can be condensed to any very considerable degree, no matter in what state. We must then conclude, *à priori*, that under high pressures the Boyle-Mariotte Law is inapplicable.

2. The researches of Rumford, which date from last century, relating to the density of the combustion gases of powder; also the researches of M. Natterer, on the compressibility of gases like oxygen, the oxide of carbon, hydrogen, and air, made in the years 1850 *et seq.*, are completely in accordance with the conclusions stated above, and show that under high pressure gases are endowed with a positive compressibility analogous to that of solid or liquid bodies. I mean by positive compressibility that property by which bodies, in proportion to the increase of pressure, diminish in volume less rapidly than the pressures increases. In cases where the Boyle-Mariotte Law is found exact, the product pv of the pressure p into the volume v remains constant; and $\frac{d(pv)}{dp} = 0$. When the compressibility is positive this product pv increases together with the increase of the pressure, and consequently $\frac{d(pv)}{dp} > 0$.

3. At the same time MM. Despretz, Regnault, and many others, have conclusively demonstrated that gases like carbonic and sulphuric acid, which are transformed into liquids under considerable pressure, possess a negative compressibility, so that for them $\frac{d(pv)}{dp} < 0$. M. Regnault has found the same negative compressibility for air, nitrogen, and the oxide of carbon under pressures higher than that of the atmosphere as far as thirty. It necessarily follows that if the data of MM. Regnault and Natterer are correct, the negative compressibility of air becomes under a certain pressure above thirty atmospheres, equal to zero, and therefore positive. When $\frac{d(pv)}{dp} = 0$, Mariotte's Law is verified. Consequently there is a certain pressure above thirty atmospheres under which Mariotte's Law is applicable; under pressures below that point the compressibility is negative; under pressures above the same, it becomes positive and remains so to the end.

4. Although there is no doubt that the Boyle-Mariotte Law is not rigorously applicable even under moderate pressures, yet the prevailing doctrine, so rich in instruction on the nature of gases, which deduces all their properties from the *vis viva* which animates their molecules, admits the supposition that in the rarefaction of gases the distances between these molecules increase to

such an extent that their mutual attraction is destroyed; and in this case gases comply exactly with the Boyle-Mariotte Law. On this hypothesis the law becomes a limit towards which every gas tends in proportion as the distance between its molecules increases, and in proportion as their *vis viva* and the rapidity of their motion increase. That idea finds no support in facts. If it were accurate, then, at a certain high temperature, gases, especially those whose density is not great and whose particles are endowed with a very great rapidity of movement, ought to conform rigidly to the Boyle-Mariotte Law; but this is inconceivable, leading, in fact, to the paradox examined above. Moreover, the researches of M. Regnault on hydrogen, the lightest of gases, have shown the very opposite, hydrogen being positively compressible under pressures only a little higher than that of the atmosphere. It indubitably follows that in reference to the diminution of a mass of a gas filling a certain volume and the increase of the rate of movement of the molecules, we cannot expect a rigorous compliance with the Boyle-Mariotte Law. But we must make certain of finding positive errors, and the following are the grounds which urged me to have recourse to new experiments in reference to the application to gases of the law with which we are dealing.

These grounds were explained by me in 1872; since then similar ideas have been published by many others. Nevertheless, so far as I know, these ideas are far from having been generally adopted into science. It is evident that to combat an established opinion, a single *à priori* conclusion is insufficient; new researches are necessary, all the more that doubt may arise in the mind on the accuracy of the data of experiments, not only such as those of Rumford and M. Natterer, but even those which the celebrated experiments of M. Regnault have established. On examining critically the processes of the last-named eminent experimenter, we may not be able to discover any cause to explain the positive deviations which he has obtained for hydrogen, although it is possible to admit that the negative deviations depend on some defects in experimenting. New experiments and researches thus became indispensable; above all, for the purpose of verifying the data of MM. Regnault and Natterer, following methods not less precise than those which these observers employed. Thus it was specially necessary to experiment on the compressibility of gases under pressures less than that of the atmosphere, seeing that until 1872 there had been no accurate researches on this point.

I distributed as follows the work undertaken by me in 1872:—I commenced with pressures less than the pressure of the atmosphere, and I passed from that to pressures which exceeded those employed by M. Regnault. For the latter purpose I devised in 1872, and have now constructed, a compound manometer, containing alternate columns of mercury and of water, and permitting the measurement of exceedingly great pressures by means of a large number of very low columns of mercury. However, at present I shall not dwell upon this side of the researches, seeing that the experiments are still being carried on; I shall only endeavour to explain the main points of the practical processes, and the results obtained under small pressures. The first experimental researches made by me on the compressibility of air under pressures less than that of the atmosphere were made by means of very simple apparatus. Imagine a vessel A terminated above and below by tubes. The upper tube is always in communication with a syphon barometer, or, as I call it, a baro-manometer. In this apparatus the height of the column of mercury measures the elasticity of the gas in the vessel. It is easy to make the volume of gas in the baro-manometer remain the same all the time, notwithstanding the variety of pressures. To accomplish this it is only necessary to arrange so that we may at pleasure increase or diminish the quantity of mercury in the baro-manometer. The lower tube of A serves to introduce and to withdraw the requisite quantities of mercury. It is not necessary to see the height of the mercury in the vessel; the mercury here serves only to measure the volumes, and consequently, if we close the feeding-tube by means of a cock, and if by means of an emptying tube we allow all the mercury in the vessel to escape, we may ascertain the capacity of the whole reservoir; by emptying only a part of it we may ascertain the volume of the gas at each moment. Thus the weight of the mercury, directly observed by means of a balance, gives immediately, in all my researches, the volume occupied by the gas in each particular case. The first experiments made in 1873 on air, showed me that air under pressures lower than that of the atmosphere possesses a positive compressibility, and that the smaller the pressure the more are the divergences presented by air from Boyle's Law increased. The first apparatus was constructed in a very simple manner; some errors might be suspected, and this is why I am not confident of the results obtained. I therefore constructed a second and a third apparatus, modifying successively not only the dimensions, but also the very construction of the details of the apparatus. I then arranged a fourth apparatus, by means of which, with Michel Kirpitcheff, whose death is a sensible loss to Russian science, I made numerous observations. The report of these experiments was given by me in 1874 to the Russian Chemical Society, and printed in the *Bulletin* of the St. Petersburg Academy of Sciences. The experiments themselves are described in considerable detail in vol. i. chap. 9, of my work "On the Elasticity of Gases." It is impossible to describe in an article the great number of particulars which belong to these researches; we are limited to the more important details which I have introduced into this inquiry, as also into my subsequent investigations.

The normal metre and normal kilogramme which I employed were compared with the Paris standards at the Conservatoire des Arts et Métiers in concert with M. Tresca; their sub-divisions were then carefully verified.

I had to work a long time at the construction of the barometer, and I found out a new process for constructing this apparatus, which consists specially in terminating the end of the barometric chamber by a capillary tube bent downwards. By means of this tube it is possible to expel the last traces of gas which remain in the vacuum, and thus to show the experiment of obtaining an absolute vacuum, *i.e.*, to construct a barometer such that with the diminution of the volume of the chamber the indications do not vary. The construction of two barometers with a common chamber and a single descending capillary tube affords an easy means of obtaining with the greatest precision the determination of the feeblest tensions in the barometric chamber. It is only necessary to direct the telescope of the cathetometer to the top of the column of mercury in one of the barometers when the mercury in the other is at its maximum height, and when the volume of the vacuum is very small; then pouring out the mercury contained in the other barometer, and thus diminishing the pressure which acts upon the vacuum, we may increase its capacity. Then the least quantity of gas contained in the vacuum will give an increase of height in the barometer observed. By constructing the barometer with the greatest care, and filling it with mercury distilled according to Weinhold's process, it is possible, as our numerous researches with M. Hemilian have proved, to obtain a perfect barometer requiring no correction for the tension of air which may remain in the vacuum. This result is obtained solely by means of the capillary tube referred to above. The process shows, moreover, the possibility of constructing barometers without boiling the mercury and without removing them from the position which they are ultimately to occupy. Here then is an undoubted improvement in the construction of an apparatus so important as the barometer, in a great number of physical researches.

Next a very long time and a great number of trials were necessary in order to attain the desired accuracy in measuring heights. I always employ the comparative method, consisting in placing beside the height to be measured a standard metre, well tried beforehand in all connections. My normal measures are generally in the form of tubes, in the inside of which is introduced water which enables me to appreciate at each moment the temperature of the measure, and if necessary even to change it. The telescopes of all my cathetometers are fitted with micrometer eye-pieces, carefully constructed by our engineer, M. Brauer, justly noted for his long residence at Pulkova, and for the construction of a great number of astronomical and magnetic apparatus. Besides the central cross-wire the micrometer eye-piece is fitted with one movable wire, or still better, with two movable wires. The fixed wires passing by the optical and geometrical centre of the telescope fitted with a level sensible to about 2–3″ are directed towards a point of the object whose height is to be determined. Then both telescopes fastened to the same cathetometer, or better to two small separate cathetometers, are directed to the normal measure arranged at the side, and the double movable wire serves to determine the distance of the fixed wire from the nearest lines of the normal measure. This last is placed at such a distance from the column whose height is being determined, that the measure and the object may be distinctly visible without changing the position of the eye-piece. Every variation in the position of the eye-piece may derange the position of the optical centre; this is why latterly I use exclusively cathetometric telescopes, in which the distance of the eye-piece from the objective cannot undergo any change. On the other hand, it is possible to move the lunette if the cathetometer itself is in the rest where it is fixed; which is not seldom necessary in practice. By using a considerable magnifier and an illuminator of the columns of mercury very carefully combined, it is possible to observe the columns with a precision carried to thousandths of a millimetre; so that the error in appreciating the height does not exceed 0·01 mm. Everyone who has worked with the ordinary cathetometers and who has used their scales for measuring heights, knows that the accuracy of the measurement by means of such apparatus never exceeds $\frac{1}{25}$ mm., and that often he makes errors which reach tenths of a millimetre. It is sufficient to refer to the variations of temperature infallibly due to the presence of the observer. In the construction applied by me, these reasons, as well as many other causes of error, do not exist at all.

Although for the barometers and the baromanometers I always use tubes of large diameter, exceeding 17 and very often even 20 millimetres, nevertheless I have thought it proper to verify the capillary depression of the mercury depending on various diameters of the tube and various heights of the meniscus. A very extensive research has been made in my laboratory by Mlle. Goutkovsky, and the results which she has obtained have obliged me to change the data which we possess on the depression of mercury. I cite one example from many which are in my work on "The Barometric Levelling and on the Application of the *Syssotomer* to that purpose." The diameter of the tube being 8·606, and the height of the meniscus—

0·6 0·8 1·0 1·2 1·4 millimetre,

the depressions are—

0·162 0·235 0·312 0·380 0·458 ,,

numbers differing from those generally adopted, according to which for a height of the meniscus 1·0, there ought to be a depression of 0·460 for the diameter 8·606.

DE MENDELEEFF

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

THE SUSPECTED INTRA-MERCURIAL PLANET.—M. Leverrier has issued an ephemeris of positions of the hypothetical planet, interior to Mercury, derived apparently from the two orbits to which reference was made last week as representing the observations upon which the general formula was founded, with equal precision, and if the planet should not be met with in transit across the sun's disc between March 21 and 23, use may be made of M. Leverrier's ephemeris to examine with large telescopes the positions of the greatest elongation westward in the two orbits. The differences of right ascension and declination from the sun about these times are thus given:—

	ORBIT I.		ORBIT II.	
	Diff. R.A.	Diff. decl.	Diff. R.A.	Diff. decl.
	m.	°	m.	°
March 28	− 38·4	− 2·6	− 38·8	− 2·6
29	− 40·4	− 2·6	− 42·0	− 2 8
30	− 40·4	− 2·8	− 44·8	− 2·9
31	− 39·2	− 2·6	− 45·2	− 3·0

The observation of Decuppis at Rome in 1839, one of the five utilised by M. Leverrier, was communicated to the Paris Academy of Sciences on December 16 in the same year. It is thus noticed in the *Comptes Rendus* of that sitting: "M. Decuppis announced that on October 2, while continuing the observations which he has made upon the spots of the sun, saw a black spot, perfectly round, and with well-defined contour, which advanced upon the disc with rapid motion, so that it would have traversed the diameter in about six hours. M. Decuppis thinks that the appearances which he has observed can only be explained by admitting the existence of a new planet." The observation is reproduced here, as it appears to have escaped the notice of several writers who have recently entered upon this subject. Haase mentions it, but does not give particulars.

The observation by Mr. Joseph Sidebotham at Manchester, on March 12, 1849, was communicated to the Literary and Philosophical Society of that city, April 1, 1873, and will be found in the *Proceedings*, vol. xii. p. 105. "A small circular black spot" was "watched in its progress across the disc for nearly half an hour," by Mr. Sidebotham and Mr. G. C. Lowe, also a member of the same society.

D'ARREST'S COMET.—If this comet is not detected before moonlight interferes in the mornings, it may probably be observed in the middle of the ensuing month, where the sky is very transparent down to the eastern horizon; it will then rise rather more than two hours before the sun, and the intensity of light will be greater than when it was last seen by Prof. Schmidt at Athens in December, 1870; still its distance from the earth will be considerable (1·7). When theoretically brightest, in May, observations may be made at the observatories of the southern hemisphere. At the Cape, Melbourne, and Sydney, the comet will rise more than four hours before the sun; the perihelion passage takes place on May 10. The following positions will sufficiently indicate its course about that time:—

At Greenwich Noon.

	R.A.	N.P.D.	Distance from Earth.
	h. m. s.	° ′	
May 2	23 18 16	91 17·0	1·670
6	23 32 13	90 31·9	1·659
10	23 46 5	89 47·8	1·650
14	23 59 51	89 4 8	1·642
18	0 13 28	88 23·3	1·635
22	0 26 56	87 43·6	1·629
26	0 40 14	87 5·8	1·624

The intensity of light remains sensibly the same during this period. In August and September next observations may be practicable with very powerful instruments, as the comet moves from Taurus into Orion.

According to the elements of M. Leveau, who has continued the investigations on the motion of D'Arrest's comet, commenced on its first discovery in the summer of 1851 by M. Villarceau, the dimensions of the orbit in 1877 are as follow:—

Semi-axis major	3·5414
Semi-axis minor	2·7565
Semi-parameter	2·1456
Perihelion distance	1·3181
Aphelion distance	5·7647

The period of revolution in the ellipse of 1877 is 2434·2 days, or 6·664 years; it has been lengthened 104 days since 1851, by the effect of perturbation from the action of Jupiter, the principal disturbance of its motion having taken place in the spring of 1861, when the comet approached the planet within 0·36 of the earth's mean distance from the sun.

TOTAL SOLAR ECLIPSES.—It might be worth while to collect together and discuss the various notices of the total solar eclipses of 1386, January 1, and 1415, June 7, in the same manner that Prof. Schiaparelli and M. Celoria have done with the eclipses of 1239 and 1241. The eclipse of 1415 in particular was a very notable one from the large excess of the moon's augmented diameter over the diameter of the sun; as Baron de Zach states, "plusieurs historiens et presque tous les astronomes en ont parlé." Both eclipses were total at Montpellier, not a common occurrence at a particular place in an interval of only twenty-nine years.

METEOROLOGICAL NOTES

MEAN ATMOSPHERIC PRESSURE IN RUSSIA IN EUROPE.—A paper on this subject, by M. Rikatcheff, appeared some time ago in the *Repertorium für Meteorologie*. The work is based on monthly averages for various terms of years for thirty places in Russia, to which are added the averages for thirty-three places situated in other parts of Europe. A valuable part of the paper is that which gives the details of the observations at each place,